DANIEL M. SIEGEL, **Innovation in Maxwell's Electromagnetic Theory: Molecular Vortices, Displacement Current, and Light.** Cambridge: Cambridge University Press, 1991. Pp. x + 225. ISBN 0-521-35365-3. £30.00, $49.50.

For those familiar with Siegel's work this book will contain few surprises, if only because much of the material has appeared in print elsewhere. In this expanded and restructured form, uninitiated readers will find a coherent, readable account of a fascinating episode in nineteenth-century physics. The focus is upon two paradigmatic examples of 'theoretically motivated innovation' (p. 2): the introduction of the displacement current and the assimilation of optics into the resulting electromagnetic theory. Both were achieved during the year which saw much of the writing and all of the publication of the papers 'On Physical Lines of Force' (1861–62). The four instalments presented, in Maxwell's words, applications of 'The Theory of Molecular Vortices'.

Discussion is located in a context of competing nineteenth-century electromagnetic theories with charge-interactionists pitted against the field-primacy school to which James Clerk Maxwell was allied. In new material Siegel examines Maxwell's uneasy synthesis of the methodological traditions, if such they were, of Edinburgh (represented by Forbes and Hamilton) and Cambridge (Herschel and Whewell). William Thomson, Maxwell's mentor of the early 1850s, is portrayed as adhering to a methodological middle way. This rather simple tripartite scheme of available 'methodological options' (p. 28), complete with a somewhat anachronistic characterization of the Cambridge style as 'deep theory' (p. 19), provides a loose framework for the rest of the book.

But the key to understanding the central 'nexus of innovation', Siegel maintains, is a painstaking analysis of the elaborate mechanical vortex model. The analogically-motivated 'mechanical images' of fluid flow in 'On Faraday's Lines of Force' (1855–56) were, in Siegel's view, essentially illustrative: preliminary to the complete, explanatory and realistic mechanical theory of molecular vortices which was to follow with 'On Physical Lines of Force'. Much of this is well-known in outline. Siegel concludes, however, that Maxwell had a stronger ontological commitment to molecular vortices than has been appreciated, this commitment motivating substantial articulation of the model in a manner which was in essence both mathematically and mechanically consistent. Nevertheless, from 1864, with the publication of 'A Dynamical Theory of the Electromagnetic Field', Maxwell began a gradual retreat from the detail of the model. Later sections of the book deal with the implications of this withdrawal, the wider reception of Maxwell's work, and its subsequent impact.

There is an undoubted clarity to this account. A pragmatic decision to 'translate' most equations into vector/tensor form, not in itself guaranteed to ingratiate Siegel with historians of mathematics, enables the dubious assimilation of Maxwell to a modern viewpoint: in so far as we are shown the motivation leading to 'Maxwell's equations' (*pace* Hunt), it is against the backdrop of a 'standard account' culled from the physics texts of the 1950s and 1960s. The fourth chapter, in particular, demonstrates at length just how Maxwell differed from this standard, an exercise which may be of more immediate interest to physicists nurtured on such texts than to historians wishing to comprehend Maxwell within his own environment: clarity has been achieved partly at the expense of historical richness by an author who has intentionally restricted his attentions to the 'directly relevant contextual material' (p. 3).

Although provided with the epithet 'mechanical engineer of the magnetoelectric medium' (p. 67) Siegel's Maxwell was insulated from the world of telegraphic engineering and undisturbed by the vibrations of a mechanized Victorian Britain. An isolated figure, impervious to social and academic forces, he was not, apparently, the widely-read and active correspondent that we know from Campbell and Garnett. Was Maxwell unperturbed, for example, by Macquorn Rankine's attempt once again to justify his own use of 'the hypothesis of molecular vortices' before the Glasgow Philosophical Society, in an essay clothed as a methodological discourse yet fortuitously designed to coincide with the publication of 'On Physical Lines'? In short, might not some of the skills directed by Siegel towards painstaking numerical, symbolic and graphological analyses (Appendix 2) have been just as usefully applied to such other issues, particularly given the luxury of a full-length book? Certainly we need to respect the equations, but we can always gain by looking outside the text to see science in context.

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